struts, even if arranged as he first conceived them to be, could not have failed by flexure during the wildest hurricane.

4. Sir George finds fault with the connection of the brackets, and "can hardly imagine that trains could be run through at speed." I should have been pleased to have explained the connection to Sir George, but he has not sought to know anything about the details of the bridge, and, I am sure, would be much puzzled to give your readers even the vaguest possible description of the connection, which he nevertheless stigmatises as "not very perfect."

In conclusion, as Sir George has not done so himself, I would warn any young student who may have read the investigation contained in the appendix to the first letter, that the methods therein proposed would lead to an over-estimate of the strength of struts of ordinary proportions by from 200 to 300 per cent. This warning is the more necessary, as the general tenour of Sir George Airy's letter might make a student imagine that he erred, if anything, in the direction of excess of caution, whereas the application of the principles laid down by him would, in the case of the Forth bridge, result in the compression members being made only one-third of the strength considered expedient by Mr. Fowler and myself. B. BAKER

2, Queen Square Place, Westminster, S.W.

P.S.—It may interest some of your readers to know that the maximum force recorded during recent storms by our wind pressure plates at the Forth has been 20 lbs. per square foot, upon the small and light plate having an area of 2 square feet, and 12½ lbs. upon the large and heavy one, with an area of 300 square feet. The same ratio holds good down to pressures of 2 lbs. per square foot, and it appears pretty certain that the higher blasts are of such momentary duration and of such unequal distribution, that even a small sized railway bridge could never experience ordinary anemometer pressures. Other reasons for a reduced pressure on a large surface have been advanced by Dr. Siemens in a recent number of the *Comptes Rendus*. Nevertheless, in this instance of the Forth bridge we have assumed that a 56 lbs. hurricane will act simultaneously over the whole width of the Forth, with a resultant lateral pressure of no less than 8000 tons upon the main spans. We have further assumed that the said hurricane might blow down one side of the Forth, whilst a dead calm prevailed on the other side, and have even provided for the twisting action upon the piers and superstructure due to a 56 lbs. hurricane blowing *up* the Forth on one side, and *down* it on the other. To ascertain what lateral pressure a 56 lbs. hurricane would cause, we tested, both in air and in water currents, a large model of the bridge, with cross-bracing complete, and ascertained its equivalent in square feet of flat surface. Under any of the conditions of wind pressure enumerated above, combined with any distribution of the rolling load, the resultant stresses upon superstructure, holding down bolts and piers will be far within the safe working limits as determined by our experiments upon the respective materials.—B. B.

Altitude and Weather

IN NATURE, vol. xxvii. p. 176, you notice the remarkable warm and dry weather September 21 last on Ben Nevis, during an anticyclone, and, as at the foot the air was relatively cold and humid, you see in the heat and dryness on the mountain an effect of descending air currents. In this you are quite right, but I do not think you are right in estimating that this air was saturated at a certain height above Ben Nevis. The fact is this: the increase of temperature from a certain height above sea-level to the latter being *de facto* much less than the dynamical increase of a stratum of air, due to compression sinking down, a downward current of air will be generally warm, and relatively dry. It does not matter if it sinks along the slopes of mountains (as the foehn), or vertically, as modern meteorology considers it to be the case in anticyclones. There is only one great difference: the air currents down a slope may be, and often are, very violent, and only when they are so, their relative heat and dryness are felt, while the downward currents in an anticyclone are so gentle that they are seldom felt or directly registered, and that mostly the thermometer and hygrometer are our only means of detecting them. On account of their slow motions, the effect of these downward currents during anticyclones is little felt in valleys and plains, as (1) they are even more retarded near great land surfaces; (2) in the colder time of the year, especially when the ground is covered with snow, the radiation from the soil lowers the temperature of the lower strata. Thus during anticyclones in winter a very low temperature is generally experienced in plains and valleys, due to radiation, and a very high temperature and low humidity on isolated mountains, due to descending currents of air.

These conditions are best realised during protracted and considerable anticyclones, and it was Prof. Hann's merit to have explained this fact.[1] The exceedingly protracted anticyclone of December, 1879, in Central Europe, was especially favourable to the proof of the existence of descending currents, as the cold was great in the valleys, even in the high ones, like the Engadine and the Davos, but the air was warm and very dry on isolated mountains. An example from the best mountain observatory of that time, the Puy de Dôme, and the foot of it, will suffice, nine days, December 20-28, 1879, at 6 a.m.

	Feet.	Temp. F.	Relative humidity	Amount of Cloud
Puy de Dôme,	4813	38·8	38	1·3
Clermont (base)	1273	8·2	91	0·7

There is all reason to think that in these days there was no saturated stratum of air even considerably above, say the Puy de Dôme.

I must remark that Prof. Hann, in his last work, "Der Föhn in Bludenz," does not sustain his former opinion that great precipitations on the windward side of mountains is necessary to the appearance of a foehn on the leeward side. His opinion now is, that a considerable barometric gradient and the drawing in of air from considerable heights are alone necessary, for even if the air on the mountains is not abnormally warm, it will come down warm and relatively dry. A. WOEIKOFF

Ofizerskaja, St. Petersburg, December 15-27, 1882

The Fertilisation of the Speedwell

I FEAR that Dr. H. Müller's passage in Schenk's "Handbuch" would occupy too much space to be given here in full; but I can condense what he says into a few lines. Dr. Müller takes the *Veronica chamædrys* as representing a type of flowers in which the anthers have to be brought into a position to strike the body of the insect by the action of the insect itself. He finds the same arrangement in the *V. urticæfolia*. These flowers are visited by insects of various kinds, but their structure is, he thinks, explained only by what takes place when they are visited by *Syrphidæ*. When one of these insects visits such a flower, it hovers for some seconds before it, then settles upon the lower lobe of the corolla, without noticing the style which is coloured like the corolla, and which is now under the insect's body. It then crawls higher to reach the nectary, and in doing so bends down the stamens—which are also coloured like the corolla—until the anthers strike against the under part of the insect's body. The pollen thus obtained is carried to another flower, and brought into contact with the stigma when the insect first alights; and fresh pollen is again obtained by the attempts to reach the nectary. Dr. Müller either knows from observation or assumes that in the *V. chamædrys* anthers and stigma are mature at the same time. He attaches importance to the fact that both stamens and style are coloured like the corolla, and therefore appear to escape the observation of the insect; and the thinness of the base of the stamen is also noticed by him as one feature in the adaptation of the flower to the visits of *Syrphidæ*. He does not refer to the looseness of the corolla. Mr. Stapley's suggestion that this may play some part in the work of cross-fertilisation is an ingenious one, and calls for further research.

As to the *V. hederæfolia*, Dr. Müller mentions it as one of the plants that have a tendency to keep their flowers half-shut in cold and rainy weather, and thus to become cleistogamous.

I am sorry that I misunderstood Mr. Stapley's first letter upon any point; but he has misunderstood mine also, if he thinks I was not aware he wished to call attention "to the *adaptation* of the flower for cross-fertilisation." I wrote as briefly as I could, and naturally assumed that he would understand I was not thinking merely of the fact that Diptera drew down the stamens. ARTHUR RANSOM

Bedford, December 23, 1882

THE SACRED TREE OF KUM-BUM

THE dissipation of illusions is always a little painful, even after repeated experience of the process. I must confess, then, to some feeling of injury at learning from Mr. Keane's interesting review in NATURE, vol.

[1] *Zeitschr. für Meteorologie*, p. 129, 1876.

xxvii. p. 171, that Huc's "tree of ten thousand images" is nothing more than a common white lilac. Myths of this kind I have generally found to have some substratum of fact at the bottom. They can be rationalised, and mere explosion does not seem to be a satisfactory way of getting rid of them.

Now our knowledge of the indigenous vegetation of China is painfully limited. An immense portion of the flora is doubtless gone beyond recovery in the cultivated districts. Remnants of the primitive, wide-spreading forest remain, however, in the precincts of temples and monasteries, and these woods have always yielded novelties to botanists who have examined them. It had seemed, therefore, little short of certain that the sacred tree of Kum-bum would be something of considerable scientific interest if specimens of it could be got hold of.

The only edition of Huc at hand to refer to is Hazlitt's translation, published by Thomas Nelson and Sons in 1856. The well-known account of the tree will be found on pp. 324-6. According to Huc, the name Kum-bum, or as he spells it, Koun-boum, consists of "two Thibetan words signifying ten thousand images, and having allusion to the tree which, according to the legend, sprang from Tsong-Kaba's hair, and bears a Thibetan character on each of its leaves." Now, according to Kreitner, as quoted by Mr. Keane, "the Abbé Huc tells us that its leaves bear the natural impress of Buddha's likeness and of the Thibetan alphabet." As a matter of fact, he does not say anything like this. What he does say is as follows:—

"There were upon each of the leaves well-formed Thibetan characters, all of a green colour, some darker, some lighter than the leaf itself. Our first impression was a suspicion of fraud on the part of the Lamas, but, after a minute examination of every detail, we could not discover the least deception. The characters all appeared to us portions of the leaf itself, equally with its veins and nerves; the position was not the same in all; in one leaf they would be at the top of the leaf, in another in the middle, in a third at the base, or at the side; the younger leaves represented the characters only in a partial state of formation. The bark of the tree and its branches, which resemble that of the plane-tree, are also covered with these characters. When you remove a piece of old bark, the young bark under it exhibits the individual outlines of characters in a germinating state, and, what is very singular, these new characters are not unfrequently different from those which they replace."

Of the tree itself as Huc saw it some forty years ago, he gives the following account:—

"The tree of the Ten Thousand Images seemed to us of great age. Its trunk, which three men could scarcely embrace with outstretched arms, is not more than eight feet high; the branches, instead of shooting up, spread out in the shape of a plume of feathers, and are extremely bushy; few of them are dead. The leaves are always green, and the wood, which is of a reddish tint, has an exquisite odour, something like that of cinnamon. The Lamas informed us that in summer, towards the eighth moon, the tree produces huge red flowers of an extremely beautiful character."

Hazlitt's translation contains two woodcuts, one (p. 325) of the tree with its canopy, the other (p. 369) of a leaf with its markings. What the history of these illustrations is, there is nothing to show; Huc's book in the original French had, I think, none. The leaf with its markings has a by no means impossible appearance; whether the markings are like Thibetan characters, I cannot say. The outline of the leaf is not unlike that of a fuchsia, but it would not pass for a lilac.

I suspect, then, that there really was in Huc's time a tree with markings on the leaves, which the imagination of the pious assimilated to Thibetan characters. Perhaps it was the last local relic of some unknown endemic tree; in Hongkong I believe many of the endemic species are represented by but a few individuals. It may well have died and been replaced by a lilac, and the genuine markings by the fudged-up image of Budha "etched with some acid on the leaves."

It is disappointing that Szechenyi's expedition seems to have done nothing for botany. As Grisebach says, "We can only guess at the richness of the Chinese flora." Every now and then some one is induced to collect a few plants, and almost invariably they contain something new to science. A more extended knowledge of Chinese plants is now essential to a right understanding of the phyto-geographical facts of the north temperate flora. Unfortunately, the numerous Europeans who visit China are occupied with political, religious, or commercial business, with little time for subsidiary pursuits. But any of them who may chance to read these lines, may rest assured that they will be really doing a useful work by collecting and drying even a few *wild* plants in their respective neighbourhoods.

Kew W. T. THISELTON DYER

NORWEGIAN GEODETICAL OPERATIONS[1]

IN 1861 an Association was formed, under the auspices of Lieut.-General von Baeyer, having for its object the measurement of arcs of meridians, and parallels, in Europe. Most of the Continental nations joined this Association, and have carried out triangulations and spirit levellings of precision to further the objects in view. It is the intention of the Association to measure an arc extending from Palermo to Levanger in Norway, which will, however, probably be extended to the North Cape. The work before us is the report of the measurement of two base lines, and of their connection with the Norwegian triangulation which is to form part of the measurement of the above-mentioned arc. It was thought in 1862 that the existing Norwegian triangulation, supplemented and verified by some new work, would meet the requirements of the Association; but it was found, on investigation, that such was not the case, and moreover that the verifications could not be carried out, because the old trigonometrical stations could not be refound with any certainty. It was therefore decided to commence a new triangulation extending in a chain from the Swedish frontier (south of Christiana), where the chain is connected with the Swedish triangulation, to Levanger, where again a connection is to be made with another portion of the Swedish triangulation. The two base lines already mentioned are situated at the extremities of this chain of triangles, one at Egeberg, near Christiana, and the other at Rindenleret, near Levanger; both were measured during the summer of 1864, and Part I. is the report of these measurements.

The base measuring apparatus used is similar to that employed by Struve for the measurement of several base lines in Russia; it belongs to the Swedish Government, and was used for the measurement of their base lines. The apparatus consists of four cast-iron tubes, each approximately 2 toises[2] in length. One end of each tube is fitted with a small highly polished steel stud, and the other end with a "contact lever." The short arm of the contact lever terminates in a steel stud, which is intended to press against the fixed stud of the adjoining tube; the long arm moves on a scale. A measuring rod capable of varying its length to a slight extent is thus obtained, and this alteration in length can be measured with great delicacy, since the long arm of the lever greatly exaggerates it. This arrangement insures that the pressure between the rods is constant. Each tube is provided with two

[1] Publications of the Norwegian Committee of the European Association for the Measurement of Degrees. Geodetical Operations. Published in Three Parts. (Christiania, 1880 and 1882.)

[2] A toise is 2·13151116 yards as determined by Col. A. R. Clarke, C.B., R.E., F.R.S., &c.